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METEOROLOGICAL OFFICE

THE METEOROLOGICAL MAGAZINE

VOL. 90, No. 1,064, MARCH 1961

THIRD SESSION OF THE WORLD METEOROLOGICAL ORGANIZATION COMMISSION FOR CLIMATOLOGY

By R. G. VERYARD, B.Sc.

At the invitation of Her Majesty's Government, the Commission for Climatology of the World Meteorological Organization (WMO) held its Third Session since the formation of WMO at Church House, Westminster, from 1–15 December 1960. The Commission for Climatology was originally formed in 1929 in the days of the International Meteorological Organization but this was its first meeting in London. There were delegates from over thirty countries and from all five continents.

The session was opened by the Rt. Hon. The Viscount Hailsham, Q.C., Lord President of the Council and Minister for Science. In an encouraging and witty speech, Viscount Hailsham mentioned how gratifying it was that, in the field of climatology, scientists from all over the world had found a common meeting ground for discussion divorced from any political differences which might exist and he drew attention to the fact that climatology is a subject which could not be tackled successfully by an exclusively national programme but called for the closest international co-operation. In fact, throughout the session there was an excellent spirit of friendliness and without this the meeting could not have been so successful.

Following Viscount Hailsham, Sir Graham Sutton, C.B.E., F.R.S., Director-General of the Meteorological Office, Permanent Representative of the United Kingdom and a very active member of the Executive Committee of WMO, gave a welcoming address in which he remarked that, in the past, climatology had played the role of the Cinderella of meteorology but was at last coming into its own; he emphasized the importance of the dynamical approach to climatology.

After a short speech by Mr. O. M. Ashford, the representative of the Secretary-General of WMO, and the reading of best wishes sent to the meeting by telegram from the Secretary-General himself, the President of the Commission, Mr. R. G. Veryard, then gave the customary Presidential Address. His theme was "The new approach to climatology" and called for a critical appraisal of the functions, responsibilities and activities of the Commission for Climatology in view of the modern and developing three-dimensional and dynamical approach to the subject, the ever-increasing value of applied climatology and the economic importance of climatic fluctuations. The address has been published in full in the January 1961 issue of the *WMO Bulletin.*

With these formalities over, the Commission got down to business by setting up the necessary committees. Two working committees were established, one to deal mainly with organizational matters under the chairmanship of Mr. C. C. Boughner of the Canadian Meteorological Service and the other to deal mainly with technical matters under the chairmanship of Dr. H. E. Landsberg of the United States Weather Bureau. One could not have wished for two more able chairmen and the work of both committees was accomplished so efficiently that it was possible to clear their output at the plenary meetings a day ahead of schedule!

To describe in detail all the matters which were discussed during the session and the outcome of such discussion would require too much space, so only the more important items will be mentioned; fuller information will be obtainable in the "Abridged Final Report of the Third Session of the Commission for Climatology" to be published by WMO.

The item on which most time was spent was the "WMO Guide to Climatological Practices". This is one more of a series of handbooks which are being issued by WMO to provide advice and guidance to meteorological services, particularly those in newly developing countries. An introductory chapter and chapters on climatological organization, climatic elements and their observation, climatological data (collection, scrutiny, storage and supply), the use of statistics in climatology, data processing by machine methods, microclimatology, CLIMAT reports, and publication of climatic data had already been prepared and issued to members of the Commission. Other chapters, some of which had not been completed, on descriptive climatology, marine climatology, the climatology of the free atmosphere, and the application of climatological data were available in draft form. All chapters were thoroughly examined and of those already prepared two were held back from publication pending further revision—the chapters on the climatology of the free atmosphere and on applied climatology—but it was agreed that, for the time being, the material could be issued to meteorological services for information. Many amendments, affecting most chapters, were considered to be necessary and these were divided into two categories. Those in the first category, regarded as mainly editorial, would be taken care of by the Secretariat. The other amendments, requiring more time for study, would be referred to a new working group and the Commission decided to authorize the President to approve such proposals of the working group as he considered to be relatively straight-forward and non-controversial; otherwise amendments submitted by the working group would be circulated to members of the Commission for comment or referred for final decision to the next session. Thus it may be some time before a complete and generally acceptable version of the "Guide to Climatological Practices" becomes available. However, a good start has been made and a number of chapters should be "finalized" in the very near future. It may be of interest to mention that discussion on the Guide raised the question of the definition of climatology and its sub-branches. It is hardly necessary to say that complete agreement was *not* reached and would appear to be unattainable!

Much time was also spent on a review of "WMO Technical Regulations" which comprise the practices and procedures which all meteorological services are expected to follow. Particular attention was given to those regulations covering the climatological requirements of aviation. Several amendments were

agreed and the working group on the "Guide to Climatological Practices" was given the additional task of keeping under review those regulations relating to climatology. Of particular interest was the discussion on "normals". It was agreed that the best choice of period would depend on the purpose for which "normal" or reference values were needed and also on the element concerned and on geographical factors. It was realized, however, that for the use of CLIMAT reports and for climatic atlases a uniform period is required and that the combined effect of random errors and errors due to a systematic trend or change of exposure will often be minimized if a period length of 20–30 years is chosen. The outcome was that the Commission decided to recommend the use of the period 1931–60 as a reference period for CLIMAT purposes and to set up a working group to give guidance, as required, to meteorological services in regard to the most suitable periods for specified purposes and to study such aspects of the problem as the influence of periodic fluctuations on the most suitable length of the "normal" period and the need for different periods when dealing with specific climatic problems. An item under "Technical Regulations" on which agreement was *not* reached was the definition of "station level"! This was left to the working group to study.

Another item of general interest on which there was much discussion concerned CLIMAT and CLIMAT TEMP messages and CLIMAT data for ocean areas. A request from the Anti-Locust Research Centre, London, for additional CLIMAT stations in parts of Africa and Asia met with a very favourable response and countries concerned readily agreed to take the necessary measures to meet the request. A proposal that the 50- and 30-millibar levels be added to, and the 400-millibar level dropped from, the pressure surfaces for which data are given in CLIMAT TEMP reports was agreed, as was also a proposal to include two extra groups to give the mean surface pressure, temperature and dew-point at the time of release of the radio-sonde. These proposals were referred to the Commission for Synoptic Meteorology, who were also asked to study whether it would be possible to include in CLIMAT TEMP reports, for equatorial, antarctic and other remote stations, wind groups for the surface and standard pressure levels giving the monthly mean wind vector and the steadiness of the wind, the idea being to facilitate the drawing of the isobars on CLIMAT surface and upper air charts. In view of the difficulties in many countries in connexion with the computation of values for $\overline{UU}$ (mean monthly relative humidity) in CLIMAT reports, the Commission recommended that the group be made optional and that the Regional Associations of WMO should be invited to consider "local" needs for $\overline{UU}$ and to take steps to ensure that this need is satisfied. It was agreed that $\overline{UU}$ should be retained in mailing CLIMAT reports to the Editor of *Monthly Climatic Data for the World*. In regard to the latter publication, appreciation was expressed of the fine efforts of the United States Weather Bureau in producing this very valuable summary of CLIMAT and CLIMAT TEMP data and arrangements were agreed for improving the verification of the data and for notifying in confirmatory messages the monthly rainfall amounts and the number of days on which mean values for each element are based.

Special attention was given to statistical requirements and methods in climatology and also to modern techniques for data processing, publication and storage. In regard to the former item, the Commission studied an excellent report by Dr. H. C. S. Thom, who had been the chairman of a working group on the subject. It was decided that the material was best suited for publication in a "WMO

Technical Note" and the working group was re-established with the job of expanding the draft to include worked examples—the Secretary-General to arrange for publication in due course. The Commission also decided to establish a working group on data processing by machine methods and endorsed a suggestion of the Regional Association for Europe that a seminar on machine methods be held in 1962. It was considered that whilst a complete international standardization of meteorological punch-card layout was not desirable, a partial standardization as regards the contents of the cards would have advantages for international exchange and a recommendation was passed indicating which elements should be contained in specified punch-cards.

The question of climatic atlases has now become a regular item on the agenda of the Commission for Climatology, but the original aim of facilitating the provision of a World Climatic Atlas still seems far from being achieved. However, progress although slow is being maintained. The publication, as an important step towards the production of world maps, of regional climatic maps according to WMO specifications was considered to be a matter of urgency. In this connexion, Prof. S. P. Jackson of the Witwatersrand University, who was present at the meeting and who is engaged on the preparation of climatic atlases for Africa, was able to give the Commission the benefit of his experience, and the Commission decided to request that the Regional Associations should make arrangements for regional maps to be prepared and published as soon as possible. To provide guidance on both technical and organizational aspects, for example, the selection and construction of base maps, the Commission decided to re-establish a working group on climatic atlases and urged that it should be given favoured treatment with respect to the financing of a meeting. It was also agreed that, particularly to meet aviation requirements and to facilitate studies of the general circulation, climatic atlases of the free atmosphere should be prepared on a world-wide scale: it was suggested that the Secretary-General should conduct an inquiry in order to collect information on existing or planned climatic maps of the free atmosphere.

On the subject of the optimum network of stations and the programme of observations required for climatological purposes there was, needless to say, considerable argument and it was generally felt that it would not be practicable to lay down criteria for all purposes. The Commission considered that there were two main problems to be dealt with under this item, namely (i) the world network needed for climatological studies on a global scale, that is, to facilitate studies of the general circulation and of climatic fluctuations, and (ii) the networks needed in the arid, tropical and polar zones for more detailed investigations, especially where lack of population, financial stringency or other reasons had prevented the establishment of an adequate number of surface and upper air stations. It was decided to set up a working group to study the problem and to make proposals for bridging the important gaps in the networks and for remedying deficiencies in the observational programmes. In this connexion, it might be mentioned that the Commission attempted to specify as far as possible the accuracy of measurements as required for climatological purposes!

Finally, mention should be made of items involving collaboration with other international organizations. One such organization is the International Civil Aviation Organization, which was represented at the meeting by Mr. R. Berggren. Arising from a discussion of climatological requirements for aviation, it was recommended that studies of icing and turbulence (in particular, clear-air

To face p. 68]

LORD HAILSHAM OPENING THE THIRD SESSION OF THE WMO COMMISSION FOR CLIMATOLOGY

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A NEW HOUSING FOR THE SENSITIVE CUP ANEMOMETER, MARK I

(see p. 89)

turbulence) along major air routes be undertaken by the countries most concerned. Then there is the International Society of Biometeorology and Bioclimatology, which was represented at the meeting by its Secretary, Dr. S. W. Tromp. Several delegates felt that, as far as meteorologists are concerned, the field of bioclimatology had been given too little attention, both at the national and the international level. It was decided to urge the members of WMO to send delegates to future bioclimatological congresses and to set up a working group to review the most important papers on human bioclimatology, to study the use of derived climatic elements in this field and the requirements for climatic classifications in dealing with problems in human bioclimatology, and to study ways and means by which meteorological services can contribute to the sound development of human bioclimatology in their respective countries. Quite a tall order! The Commission also dealt with certain requirements emanating from the Food and Agriculture Organization (represented at the meeting by Mr. S. J. Holt) and UNESCO and endorsed a resolution which was adopted by the Council of IUGG at Helsinki this year regarding the publication and exchange of meteorological data for research purposes. The Commission was particularly interested in the joint UNESCO/WMO symposium on Changes of Climate to be held in Rome in 1961 and recommended, among other things, that the Secretary-General should invite members of WMO to supply information about published and unpublished studies on changes of climate carried out in their countries so that such information could be made available in a suitable form before the symposium; it was suggested that the symposium should give attention to the possibility of using electronic computers for the analysis of long climatic series. The President of the Commission was authorized to establish a working group on climatic fluctuations at any time he might consider suitable.

During the session two afternoons were set aside for scientific discussions. One, on climatic fluctuations, was held in the conference building when papers were read by Mr. A. I. Johnson of the Climatological Research Division of the Meteorological Office and by Prof. Dr. H. Flohn of the Deutscher Wetterdienst, who made a special journey from Germany. The other was at the invitation of the Royal Meteorological Society, the latter having chosen for a special discussion meeting the subject of "Automatic methods of handling meteorological data". Social activities included a tour of their works at Harlow (including lunch and transport) arranged by Cossor Radar and Electronics Ltd., and a cocktail party at Lancaster House at the invitation of Her Majesty's Government.

That the meeting was a success was due not only to the fine co-operation of all the delegates but to the excellent work of the WMO Secretariat, Mr. O.M. Ashford, Mr. E. Hovmoller and Mr. P. Rogers, and to the Executive Secretary, Mr. C. W. G. Daking, and his staff. The facilities provided by the Church House authorities and by the Conference and Supply Department of the Foreign Office were excellent, the interpreters doing a particularly good job of work. In fact, the meeting ended with a flow of enthusiastic acknowledgements for all the help received, on which a meeting of such a kind is so dependent. Several members felt that, although WMO regulations permitted officers to serve for a second term, it was desirable that there should be a change at each session and the President decided not to stand for re-election. Mr. C. C. Boughner, the outgoing Vice-President, was elected *nem. con.* to succeed him and Dr. C. C. Wallen of Sweden was elected by a majority vote as the new Vice-President.

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AN APPROACH TO THE PROBLEM OF THE FORECASTING OF FOG CLEARANCE

By C. J. KENNINGTON, M.A.

The time of clearance of radiation fog is never easy to forecast. While the number of factors involved is so large as to preclude any rigorous mathematical analysis of the problem, certain simplifications make the following analysis possible.

G. J. Jefferson has shown[1] how to draw a temperature-rise curve for a clear morning, and in a further article[2] has suggested a method of adjusting this curve to allow for fog. The present approach was suggested to me by J. R. Martin, and is based on unpublished work (1942) of the late D. J. Horrod. My thanks are due to J. R. Martin and Dr. K. H. Stewart for considerable help in the preparation of this paper.

The temperature which will be reached at any time on a clear morning may be calculated from a representative ascent on a tephigram using the proportionate heating amounts in Table I. This table, which is due to J. R. Martin, has been produced by assuming that the "Gold-square"[3] maximum temperature will be reached by 1430 hours, taking proportionate parts of the insolation available, allowing for the sun's elevation, for each hour, and summing them progressively. It gives the insolation, in gm cal cm^{-2}, which will be received up to each hour in any month. These figures may be applied to the 1956 Meteorological Office tephigram by noting that on this form an area of 1 cm^2 represents an energy of 50 gm cal cm^{-2}.

TABLE I—TOTAL INSOLATION RECEIVED UP TO A GIVEN TIME ON A CLEAR MORNING

GMT	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
						gm cal cm^{-2}						
0500					2	4	2					
0600				3	9	14	10	3	1			
0700			3	11	19	24	20	15	8	1		
0800	1	4	10	22	33	40	33	26	17	8	2	2
0900	4	11	22	40	52	57	50	43	32	16	6	3
1000	10	20	35	57	73	81	71	62	45	26	11	7
1100	17	32	50	75	100	106	93	79	60	38	18	13
1200	25	46	67	95	120	128	116	101	77	51	25	19
1300	32	56	81	115	139	150	135	118	93	65	33	24
1400	38	66	90	134	164	171	157	138	104	73	38	29
1430	40	70	100	140	175	180	165	150	115	80	40	30

On the main part of the tephigram, F 2810A (1956), 1 cm^2 = 50 gm cal cm^{-2}

On a foggy morning, part of the incoming solar energy is reflected from the fog top; the rest is used in heating the air and water in the fog bank, and in evaporating the water. E. W. Hewson[4] has considered at length the theoretical aspects of reflection, absorption and transmission of a beam of solar energy passing through fog and cloud. He produced a table showing the effect of droplet size, water content, thickness and solar elevation on the distribution of the incident radiation. A simplification of this is shown (Table II) for droplet size 2×10^{-3} cm (considered representative of fog) and neglecting the slight effect of varying solar elevation. This shows that relatively little solar energy is absorbed, and that the percentage of energy reflected increases with depth and with water content. It is difficult to estimate the water content of a fog. If M_b and M_a are the saturation

TABLE II—PERCENTAGES OF INCIDENT SOLAR RADIATION REFLECTED, ABSORBED AND TRANSMITTED BY A BANK OF FOG COMPOSED OF WATER DROPLETS RADIUS 2×10^{-3} CM

	Water density (gm m^{-3})								
	0·1	1·0	5·0	0·1	1·0	5·0	0·1	1·0	5·0
Depth of fog (m)	% reflected			% absorbed			% transmitted		
20	1·4	12·8	40·8	0·1	0·8	3·0	98·5	86·4	56·2
80	6·0	36·1	70·0	0·2	2·7	7·4	93·8	61·2	22·6
200	12·8	57·1	82·3	0·8	5·0	10·2	86·4	37·9	7·5

These average values neglect the small effect of varying solar elevation.

mixing ratios at the actual temperature and the temperature at which the fog formed, then $M_a - M_b$ gm kg^{-1} of water must have been condensed. This is an upper limit to the possible water content, but the actual content may be less because of loss by droplets falling to the ground. Common values of $M_a - M_b$ would be in the range 0·5 to 2·0 gm kg^{-1}. Experimental values are 0·2 to 1·0 gm m^{-3} (0·3 to 1·25 gm kg^{-1}) found in thick fogs by H. G. Houghton and W. H. Radford[5], and 0·2 to 0·4 gm m^{-3} (0·3 to 0·5 gm kg^{-1}) found in advection fogs by D. Kuroiwa and S. Kinosita[6]. At 1 gm m^{-3} (1·25 gm kg^{-1}) the reflection coefficient varies from 13 to 57 per cent with depth, while at 0·1 gm m^{-3} the range is 1 to 13 per cent. Gold allowed 20 per cent for reflection from the ground in evaluating his heating figures. The total reflection from fog lying on the ground cannot be less than this, and it is suggested that 40 and 60 per cent would be average reflection coefficients for fairly thin and thick fogs respectively. Gold in deducing his figures also deducted an allowance for normal evaporation ranging from 33 to 73 per cent of the final figures. This deduction is not relevant when considering insolation at the fog top; therefore, the available insolation for clearing the fog must be increased by this amount, as well as by the 20 per cent allowed for reflection. Table III shows the insolation available for dispersing a thick fog, after allowing for all these corrections; for a thin fog the figures should be increased by half.

TABLE III—TOTAL INSOLATION RECEIVED AT THE TOP OF A THICK FOG UP TO A GIVEN TIME, ALLOWING FOR 60 PER CENT REFLECTION

GMT	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
					gm cal	*cm^{-2}*						
0500					2	4	2					
0600				3	8	12	9	3	1			
0700			3	9	17	21	19	13	7	1		
0800	1	3	8	19	29	35	31	22	14	6	2	1
0900	3	8	19	34	46	50	46	37	26	12	5	2
1000	8	15	29	49	64	71	66	53	37	20	9	5
1100	14	25	42	64	87	93	86	67	49	29	15	10
1200	20	35	56	81	106	112	98	86	63	38	21	14
1300	26	43	68	99	122	132	125	101	76	49	28	18
1400	30	51	76	115	144	150	146	118	85	55	32	22
1430	32	54	84	120	154	158	153	128	94	60	34	23

For a thin fog these values should be increased by half.

The amount of water evaporated during clearance of fog may be deduced by considering conditions at dawn and at the moment of clearance. If the dawn temperature is T_1 and that at clearance T_2, and M_1 and M_2 are the corresponding saturation mixing ratios, then the amount of water vapour present at dawn is M_1 and that at clearance is M_2. Therefore, $M_2 - M_1$ gm kg^{-1} of water must have been evaporated during clearance; part of this will have come from the

water droplets originally contained in the fog, and part from dew or other surface water. This is true both for the surface layer and for layers higher in the fog, where the additional water will have been transported upwards by mixing.

To forecast the time of clearance of fog, therefore, we must first forecast the clearance temperature. (One way of doing this is to take the surface temperature required to give a saturated adiabatic lapse rate to the top of the inversion.) The insolation required to raise dry air to this temperature can be found from the tephigram as outlined above. However, additional energy is required to warm and evaporate the liquid water content of the fog, so this insolation must be increased by the factor deduced below. The time by which this corrected insolation will have become available on a foggy morning may be read from Table III, and this is the forecast time of fog clearance.

To deduce the increase factor, we consider a layer of fog defined on the tephigram by two constant-pressure levels, and let T_α and T_β be the actual temperature at the height of this layer and the clearance temperature (in °F), and M_α and M_β the corresponding mixing ratios. C is the specific heat of air at constant pressure, and L the latent heat of evaporation of water.

Then to heat 1 kg of air from T_α to T_β requires

$$\tfrac{5}{9}\,(T_\beta - T_\alpha)\ 10^3\,C \text{ calories;}$$

and to heat 1 gm of water from T_α to T_β and evaporate it requires

$$\tfrac{5}{9}\,(T_\beta - T_\alpha) + L \text{ calories.}$$

Now the amount of water to be heated and evaporated in clearing 1 kg of this fog (strictly 1 kg of saturated air with its suspended water droplets) at T_α is $M_\beta - M_\alpha$ gm. Therefore, to heat 1 kg of fog from T_α to T_β and evaporate its liquid water content requires

$$\tfrac{5}{9}\,(T_\beta - T_\alpha)\ 10^3\,C + (M_\beta - M_\alpha)\ [\tfrac{5}{9}\,(T_\beta - T_\alpha) + L] \text{ calories.}$$

Therefore, the increase factor f of the energy required to clear the layer of fog compared with the energy required to heat an equivalent layer of dry air through the same temperature range is

$$f = \frac{\tfrac{5}{9}\,(T_\beta - T_\alpha)\ 10^3\,C + (M_\beta - M_\alpha)\ [\tfrac{5}{9}\,(T_\beta - T_\alpha) + L]}{\tfrac{5}{9}\,(T_\beta - T_\alpha)\ 10^3\,C}$$

$$= 1 + \frac{M_\beta - M_\alpha}{10^3 C} + \frac{9L}{5000C} \times \frac{M_\beta - M_\alpha}{T_\beta - T_\alpha}.$$

Now in the range $T = 28$°F to 60°F at 1000 millibars, M can be represented closely by the formula

$$M = \frac{(T-10)^2 + 541}{272},$$

where M is in gm kg^{-1} and T in °F, and this varies only slowly with pressure. Now $C = 0{\cdot}242$ and $L = 591$ at 50°F, and $M_\beta - M_\alpha$ in normal circumstances will not exceed 10, therefore $(M_\beta - M_\alpha)/10^3 C$ can be neglected, and

$$\frac{M_\beta - M_\alpha}{T_\beta - T_\alpha} = \frac{(T_\beta - 10)^2 - (T_\alpha - 10)^2}{272\,(T_\beta - T_\alpha)}$$

$$= \frac{T_\alpha + T_\beta - 20}{272}.$$

Therefore
$$f = 1 + \frac{9}{5} \times \frac{591}{242} \times \frac{T_\alpha + T_\beta - 20}{272}$$
$$= 1 + \frac{T_\alpha + T_\beta - 20}{61 \cdot 7}$$
$$\simeq \frac{T_\alpha + T_\beta}{60} + \frac{2}{3}.$$

In order to apply this factor, it is strictly necessary to integrate it over the heating area on the tephigram, and the manner in which this is carried out depends on the assumed shape of the heating area. In practice, however, the possible forms of the integral all result in answers of the form

$$F = \int f \mathrm{d}s = \frac{T_1 + T_2}{60} + \frac{2}{3} + \varepsilon,$$

where T_1 and T_2 are the actual surface temperature and the temperature at clearance, and ε is an expression in T_1, T_2 and the fog depth, depending on the shape of the heating area, but of absolute value not exceeding 3×10^{-2} in ordinary circumstances. In comparison with the minor inaccuracies of the method, this can be neglected.

In practice, therefore, the factor by which the insolation required must be increased is

$$F = \frac{T_1 + T_2}{60} + \frac{2}{3}.$$

It is interesting to note that in practical cases the range of F is from about $1\frac{1}{2}$ to $2\frac{1}{2}$ (for low and high temperatures respectively).

The author has not been able to test the method exhaustively. However, the results of some preliminary tests on data from Kew kindly supplied by Dr. Stewart are given in Table IV.

TABLE IV—RESULTS OF TEST OF THE METHOD ON FIVE FOGS AT KEW

Date 1956	Insolation required for dry air gm cal cm^{-2}	F	Insolation required for clearance gm cal cm^{-2}	Forecast clearance time GMT	Actual clearance time GMT
12 Oct.	16	2·25	36	1200	1145
13 Oct.	9	2·2	20	1000	1020
14 Oct.	7	2·2	15	0920	1000
15 Oct.	7	2·3	16	0930	0900
22 Nov.	18	1·8	32	1400	1500

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551.515.4: 551.515.8: 551.543.5

A THUNDERSTORM HIGH OVER ENGLAND

By D. P. SMITH

Examples of small areas of high pressure associated with frontal thunderstorms, known as thunderstorm highs, have been given in American publications and appear to occur over the United States of America fairly frequently. They are not regarded as common over England, but on 9 July 1959 there was an example.

At 0600 GMT a weak cold front was lying from Norfolk to Dorset, and a trough south of the front extended from the Bay of Biscay to the Low Countries. There was a shallow, thundery depression centred in the trough over the Brest Peninsula. The ascents from Brest, Trappes and Crawley showed the warm air south of the front to be potentially unstable, and at 0700 GMT thunderstorms were reported by Brest and other stations near the depression. SFERIC reports and radar echoes from East Hill and Hurn at 0800 GMT showed that storms were over the Channel and were moving north-eastwards with the upper flow.

A well defined area of thunderstorms developed near the cold front and moved quickly north-eastwards, closely following the line of the front. The first report of thunder was from Portland Bill at 0900 GMT, and by 1500 GMT a thunderstorm area of about 50 miles radius had moved north-eastwards at an average speed of 40 knots. An interesting feature of these storms is that their direction of movement, from 225 degrees, was about 25 degrees to the right of the 700-millibar winds, which Newton and Katz[1] have found to be the average direction

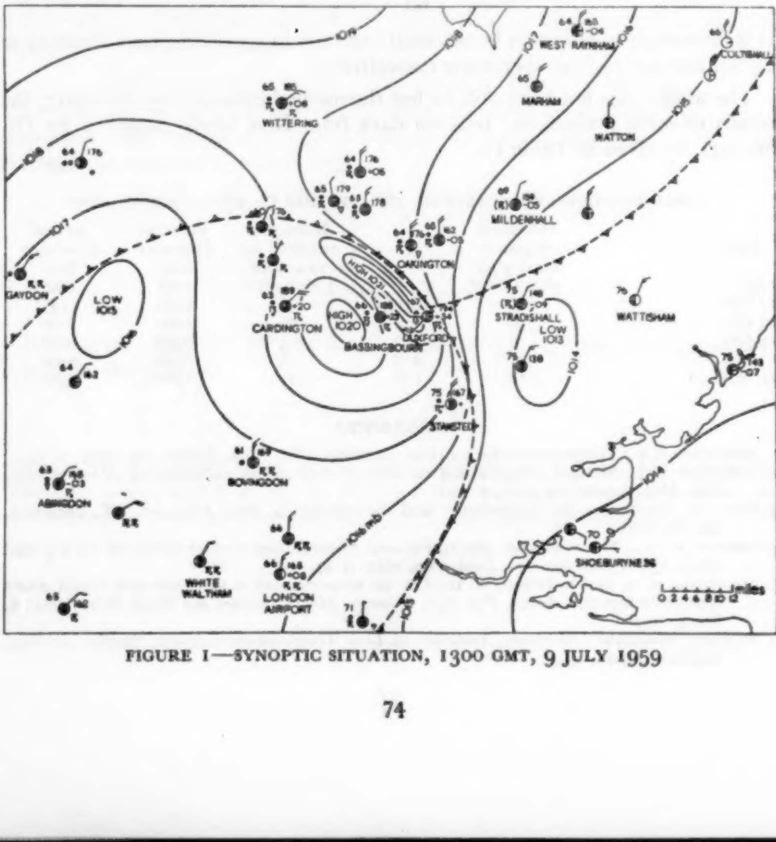

FIGURE 1—SYNOPTIC SITUATION, 1300 GMT, 9 JULY 1959

of movement of thunderstorms. Violent storms with hail were reported from White Waltham, Duxford and Stradishall.

The rapid development of these storms was undoubtedly partly due to diurnal heating. Nevertheless, the close association with the cold front is evidence of additional uplift; how this may have been created is problematical. Low-level south-east winds blowing from the depression towards the front may have been the cause. The 1200 GMT Crawley wind at 850 millibars did, in fact, show that winds over southern England were from the south-east. The cold front was, however, weak and did not extend far above a few thousand feet; it seems doubtful if this method could have caused the large-scale uplift which took place before diurnal heating was very far advanced. It appears more likely that a small mobile wave was induced on the front, perhaps by the formation of a trough extending out from the depression over France. The depression moved north-east during the day reaching Belgium by 1800 GMT and the storm centre followed a parallel course. Also, since the cold front was shallow, the air to the north of the front was similarly potentially unstable, and a minor perturbation on the cold front could have resulted in large-scale uplift.

Although there were few reporting stations on the path of the storm centre, the track is shown clearly by the hail reports collected by Ludlam[2], from whose paper the map of the distribution of maximum hailstone size (Figure 3) is mostly taken. The storm is seen to have had two surges of activity. The first lay over the Basingstoke–Maidenhead area and the second from north of the Chiltern Hills to Bury St. Edmunds. A large-scale chart of the situation at 1300 GMT (Figure 1) shows that a small high-pressure area had developed over the thunderstorm area. This was associated with the downdraught and resulting divergence of the cold air from the large concentration of storms. A pressure surge (Figure 2) marked the outward spread of this cold, diverging air, and this air had formed a pseudo-cold front against not only the warm air to the south of the original cold front but also against the cold air to the north of the front. This pressure surge was more marked than might appear from this chart, since it was offset almost immediately by a drop in pressure. All stations in the Cambridge area recorded a sharp pressure rise of about 4 millibars, with a rise at Oakington of 5·2 millibars. The passage of the pseudo-cold front, being the initial downdraught from the system, was also marked by a sharp temperature drop of 10–12°F, the onset of heavy precipitation and gusts of 30–40 knots. A further feature was the south-westerly wind which accompanied the pressure surge and with which the gusts were associated, as reported by Duxford in the 1300 GMT observation.

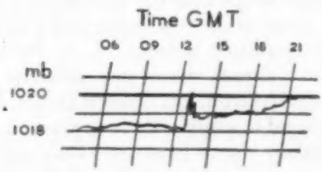

FIGURE 2—BAROGRAPH TRACE AT BASSINGBOURN, 9 JULY 1959

The records at Porton Down, Dunstable, Bassingbourn and Mildenhall have been examined over the period of the storm. These four stations were situated

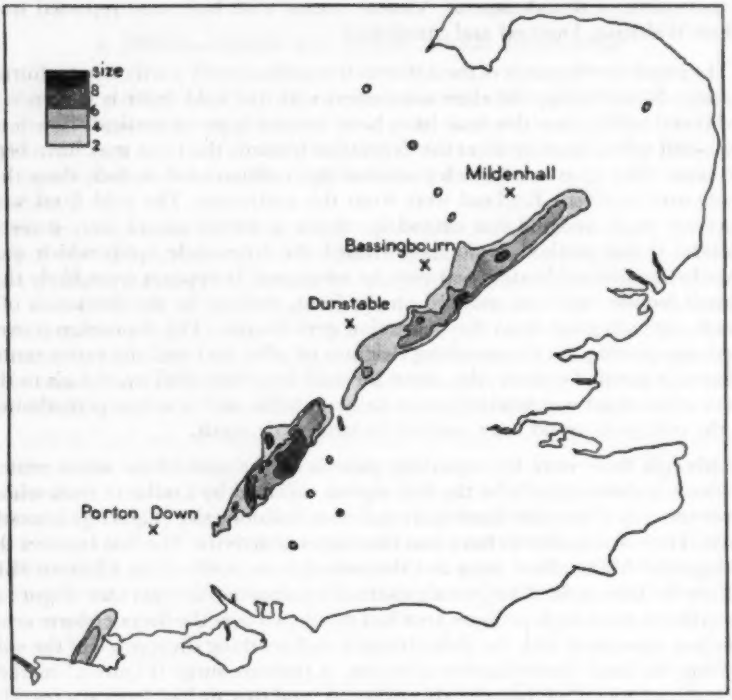

FIGURE 3—DISTRIBUTION OF MAXIMUM HAILSTONE SIZE, 9 JULY 1959

Isopleths are for stones of sizes 2, 4, 6 and 8 on the following scale: 2 = pea size; 4 = cherry size; 6 = walnut size; 8 = egg size. The small areas of hail shown to the north of Bassingbourn were probably from storms associated with the psuedo-cold front.

at about the same relative position to the track of the centre, being about 5–10 miles to the north. In relation to the two surges of activity, Porton Down was ahead of the first, Dunstable lay between the two, Bassingbourn was in the middle of the second and Mildenhall towards the end of the second. The barograph trace at Bassingbourn shows a sharp peak followed by a lesser rise and fall with a net gain of about two millibars (Figure 2). The traces at Mildenhall and Dunstable are similar except that the secondary peak is not so great; the peak at Dunstable is the lesser of the two, being half a millibar only, but at both stations a total rise of two millibars is recorded. At Porton Down the first peak was not experienced, and a simple rise of two millibars is shown. At the three stations with recording wind instruments (Porton Down, Dunstable and Mildenhall), the backing of the wind to south-west took place at the same relative time, being ahead of the secondary rise of pressure. Gusts associated with the south-west wind were 30–35 knots at Mildenhall and Bassingbourn (the latter by observation) and 16 knots at Dunstable. At Porton Down, however, the wind became almost calm. The fall of temperature and start of precipitation occurred with the secondary rise of pressure at all four stations, when the wind began veering to northerly again.

The general rise of pressure was due to the thunderstorm high cell passing over the stations. The sharp nature of the rise in pressure is typical of a front, so the pseudo-cold front had formed when the cell reached Porton Down. At this stage it appears to have had the characteristics of a normal, active cold front. At the other stations a pressure peak was recorded before the temperature drop or start of precipitation. An explanation for this peak would be a nose formed in the pseudo-cold front. As the system was in a more advanced and active state east of Wiltshire, the downdraught would be stronger and a nose more likely to be formed by surface friction. This ridge of cold and rapidly descending air recorded a higher pressure than the main downdraught which followed. The latter would be the secondary rise of pressure recorded at these stations, with the highest rise at Bassingbourn where the system was at the peak of the second surge of activity. Together with the isallobaric effect, which was considerable, the south-west wind was caused by the strong, initial downdraught in which the hail was contained; at White Waltham gusts of over 40 knots were observed. The temporary slackening of the wind speed at Porton Down would be due to the weakness of this initial downdraught, since the system was in its early stages, and the opposing isallobaric effect on the prevailing north-east wind.

The emergence of this pseudo-cold front increased the upslope motion over the whole system, since presumably the original motion still existed, but also both the warm and cold air masses on either side of the front were being lifted by the colder, descending air of the storms themselves, so that the system was partly providing its own triggering action. The area of strongest upslope motion would have been at the junction of the pseudo-cold front with the original cold front, since there three motions were combined; on the 1300 GMT chart violent thunderstorms were in that area.

The pseudo-cold front travelled through the Cambridge area at about 45 knots, and its position at 1300 GMT can be accurately determined in that area by reference to the time of the pressure surge at the various stations. Its position south of Stansted and west of Cardington is, however, rather doubtful in the absence of more detailed reports, and has been drawn to include the lower temperatures associated with the air of the downdraught (excepting the temperature at Stansted, which inexplicably dropped shortly after the 1300 GMT observation). Nevertheless, thunderstorms which occurred later over Essex and south-east England as well as at Gaydon and Cottesmore show that the pseudo-cold front was sufficiently well developed, especially against the warm air, to cause substantial lifting well away from the main thunderstorm area. Diurnal heating and orographic lifting are unlikely to have caused this secondary outbreak of thunderstorms, at least in the south, as both Southend and Shoeburyness , and later Kent coastal stations, were affected. An investigation by Ludlam[2] of radar reports on this occasion shows that at 1400 GMT storms and showers had spread outwards from the centre to the north and south on a line corresponding to the forward movement of the pseudo-cold front of Figure 1. There is also shown a small group of showers associated with the following wake depression.

The system was still active when it crossed the Norfolk coast at 1500 GMT, but the cessation of hail north of Bury St. Edmunds shows that the system was weakening. The storm probably died out over the North Sea, and SFERIC reports confirm this. During its passage, considerable damage was caused by hail on greenhouses near Wokingham, and a low-flying aircraft from Bassingbourn was similarly affected just south of the airfield.

It is interesting to note that a similar outbreak took place on the night of 10 July. A smaller pressure surge was recorded by the stations whose records are examined above, but it is probable that they were not as close to this second storm. At Kew the highest rainfall of the month was recorded on that night. Perhaps thunderstorm highs are not as uncommon in this country as is supposed.

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METEOROLOGICAL OFFICE DISCUSSION

Numerical forecasting at Dunstable

It is now two years since the high-speed electronic computer, Meteor,[1] was installed in the Napier Shaw Research Laboratory at Dunstable. Much of the machine's working-time has been used for numerical forecasting.

Opening the Monday Discussion on 17 October 1960, Mr. C. E. Wallington outlined forecasting experiments carried out at Dunstable since January 1959. After a brief introduction to numerical forecasting techniques,[2] he illustrated the basic features of the Sawyer–Bushby model,[3] which was formulated and had been ready for use for some time. As soon as Meteor was installed, the staff of the Dynamical Research Branch put the model to a prolonged test.[4] On almost every weekday from 12 January to May 1959, 24- and 30-hour forecasts were made from current midnight data. The initial data for each forecast comprised 500-millibar contour heights and 500–1000-millibar thicknesses for a network of 480 grid points covering an area extending from Nova Scotia to Russia and from Spitsbergen to North Africa. The distance between adjacent grid points ranged from about 130 to 170 nautical miles. The model included a statistically based effect of heating over the sea[5] and the end product of the computations comprised forecast charts of the two basic fields—the 500-millibar contour and the thickness fields—and the difference between these two fields, namely the 1000-millibar pattern. The computed vertical motion at the 600-millibar level was also obtained.

In view of the necessary, but generally incorrect, assumption of no changes near the boundary of the computing area, hopes for satisfactory forecasts were limited to an inner "verification" area—a rectangular network of 192 grid points covering about the same area as the Central Forecasting Office (C.F.O.) routine prebaratics, that is, the north-east Atlantic, Iceland and Europe.

The nature of the errors.—Broadly speaking, the 24-hour forecasts were encouraging. Some were very good, but to improve the general standard of forecasting we are more concerned with the faults of the technique. One of the principal faults stems from the boundary assumptions. Although most boundary effects in 24-hour forecasts are limited to peculiar distortions in the predicted contour and thickness patterns close to the boundaries, there are occasions when they do invade the verification area.

The assumption of no change at the boundary not only prevents systems from entering the computing area but also stops them getting out. Figure 1 shows a

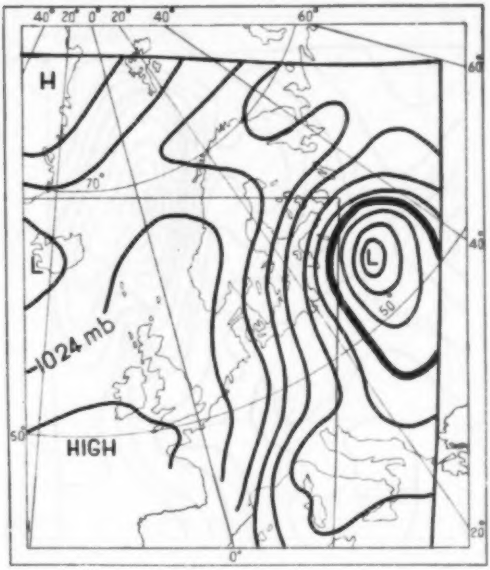


FIGURE 1(*a*)—ACTUAL MEAN-SEA-LEVEL CHART FOR 0000 GMT, 20 APRIL 1959

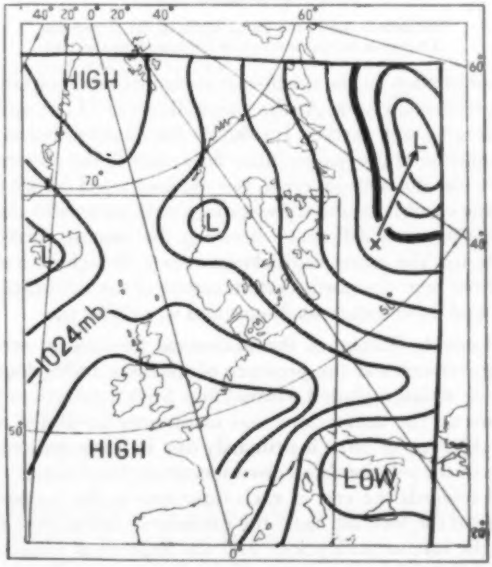


FIGURE 1(*b*)—ACTUAL MEAN-SEA-LEVEL CHART FOR 0000 GMT, 21 APRIL 1959
The arrow shows the 24-hr track of the depression.

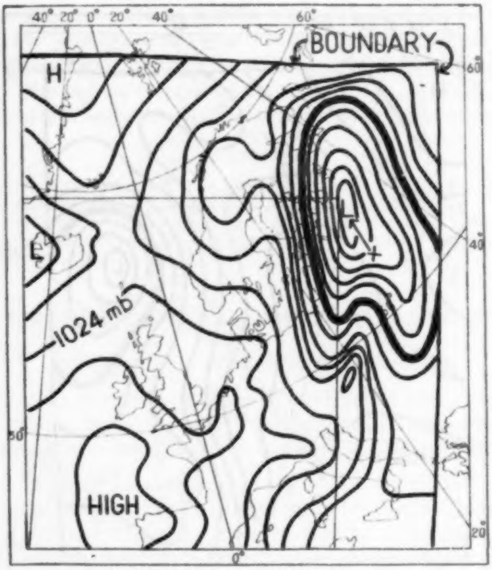
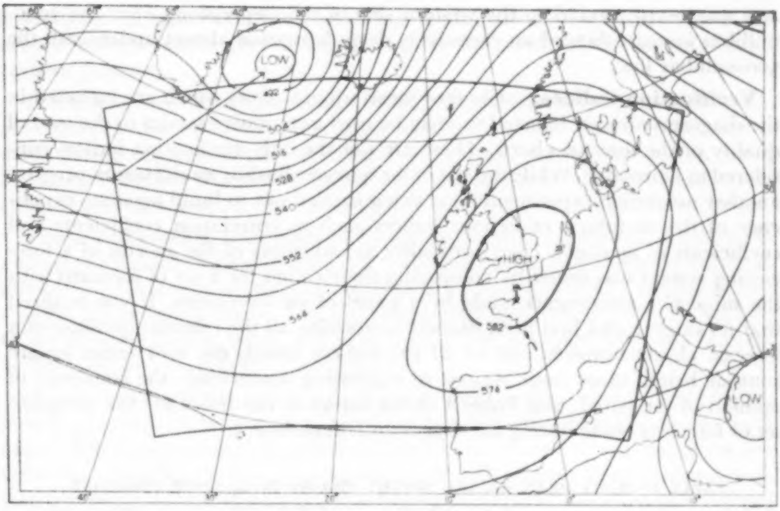


FIGURE 1 (c)—24-HOUR NUMERICAL FORECAST OF MEAN-SEA-LEVEL CHART FOR 0000 GMT, 21 APRIL 1959, USING THE TWO-LEVEL SAWYER–BUSHBY MODEL

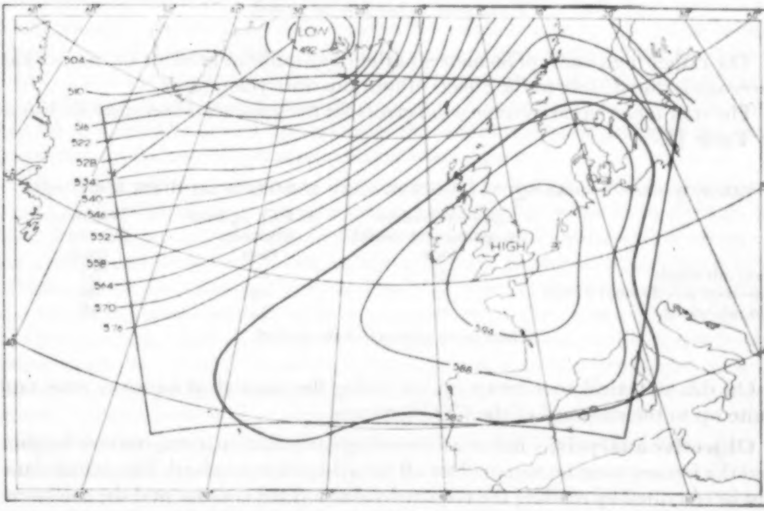
The arrow shows the 24-hr track of the depression. The error in this forecast is explained in the text.

1000-millibar prediction in the north-east corner of the computing area, at the corner of the verification area. At the 500-millibar level the numerical forecast was almost, but not quite, correct. Inevitably the model failed to predict a slight change in orientation of the 500-millibar flow close to the eastern boundary. By itself this failure was not a major error, but the associated failure to advect warm air out across the eastern boundary was more noticeable and the 1000-millibar forecast, being the small difference between the almost correct 500-millibar contour heights and the excessive thickness values, included an excessively deep depression centred near the north-eastern corner of the verification area. In fact the depression had moved east-north-east and was filling up.

Another noticeable feature of the numerical forecasting errors during the four-month experiment was the presence of spurious anticyclogenesis, particularly in situations which included a broad flow from the south or west—like that shown in Figure 2. The anticyclone was incorrectly predicted to broaden and intensify, and this type of error is primarily due to using geostrophic instead of gradient winds in the relationship between contour heights and vorticities at the beginning and towards the end of each time step in the numerical prediction process. Notice on the forecast chart the excessively strong flow from the south-west, between the intense anticyclone and the depression whose movement was forecast fairly well. With such a strong flow it is not surprising that the thickness lines were advected too far. The tongue of warm air was predicted to move so quickly northwards that it entered the circulation of the 500-millibar depression



(*a*) Actual 500-millibar contours, in decametres, for 0000 GMT, 18 February 1959



(*b*) 24-hour numerical forecast of 500-millibar contours, in decametres, for 0000 GMT, 18 February 1959, using the Sawyer–Bushby model

Verification data: root mean square height error = 143 m;
root mean square vector wind error = 30 kt.

FIGURE 2—A CASE OF SPURIOUS ANTICYCLOGENESIS

and was swept around to the western side of this depression. Thus the 1000-millibar forecast showed an excessively deep depression almost underneath the 500-millibar low.

Verification indices.—The root mean square errors which are included in the diagrams already referred to, may be used as tentative guides to the overall quality of the forecast charts. These are not the only verification indices considered at Dunstable. While the test of the Sawyer–Bushby model was in progress another numerical experiment[6] was being carried out to build up some experience of the usefulness of various indices such as correlation coefficients and coefficients of analogy. Their suitability as indicators of the success of a forecasting system was tested by comparing their values for a set of forecasts with the subjective assessments made by a panel of six forecasters. It was realized that no single index had been devised to describe all the relevant qualities of a forecast chart; however, out of all the indices tested, the root mean square contour height error came nearest to expressing numerically the consensus of opinion of the panel, and Table I shows values of this index for the complete set of forecasts made during the four-month experiment.

TABLE I—ROOT MEAN SQUARE HEIGHT ERRORS IN 24-HOUR FORECASTS

	Sawyer–Bushby two-parameter model *metres*	C.F.O. 24-hour forecasts *metres*	"Persistence forecasts" *metres*
1000 mb level	80	60	75
500–1000 mb thicknesses ...	56	58	77
500 mb level	89	75	99

There were 59 cases in the period.

The rather high value of 80 metres at the 1000-millibar level for the numerical forecasts is due mainly to the effects of spurious anticyclogenesis.

The root mean square vector wind errors for the same set of forecasts are listed in Table II.

TABLE II—ROOT MEAN SQUARE VECTOR WIND ERRORS IN 24-HOUR FORECASTS

	Sawyer–Bushby two-parameter model *knots*	C.F.O. 24-hour forecasts *knots*	"Persistence forecasts" *knots*
1000 mb winds	30	21	31
500–1000 mb thermal winds	29	24	32
500 mb winds	30	25	36

There were 59 cases in the period.

On this statistical root mean square rating the numerical forecasts were not quite up to the standard of the CFO forecasts.

Objective analysis.—Before a forecasting computation starts, contour heights and thicknesses must be obtained for all the grid points involved. This initial data can be obtained by reading the required values off the contour and the thickness charts, and punching a data tape for insertion into the machine. But this takes time. So, during the four-month experiments, an alternative system was developed. Briefly, the scheme was to feed ordinary communications tapes, containing routine data in the synoptic code, into Meteor's tape reader. The machine was then programmed to extract and store relevant radio-sonde data for a wide area. Then the contour heights and thicknesses were computed for each grid point by fitting

a quadratic formula (with some empirical modifications) approximately to the nearest radio-sonde heights and winds and to the results of the previous day's 24-hour forecast.

The testing and development of this objective analysis,[7] as it is called, was more difficult than the actual forecasting problem, but considerable progress was made during the four-month experiment and, when a second four-month test of the Sawyer–Bushby model was made during the summer of 1959, the automatic extraction of data and the objective analysis were reasonably fast and accurate.

The use of stream functions.—An attempt to eliminate spurious anticyclogenesis can be made by transforming the initial contour fields into stream-function charts.[8] In effect this transformation means adjusting gradients to take cyclostrophic forces into account. The numerical prediction process may be applied to the stream-function chart, and a forecast stream-function field may be converted back into an ordinary contour chart.

Certainly the injection of a stream function into the prediction procedure does reduce spurious anticyclogenesis, but there are difficulties in applying the technique. The computations required to turn contours into stream functions are sometimes lengthy, and it is occasionally difficult to steer these computations past inherent mathematical pitfalls. A further problem of more synoptic interest arises from the use of stream functions to represent thicknesses in the usual numerical prediction techniques. The difference between stream-function changes at two pressure levels is not a simple thickness change which can be equated easily to the thermodynamical equation for thickness changes. In effect, the appropriate stream-function equations usually include a slight approximation which weakens the synoptic link between the contour and the thickness fields. Therefore, at the 1000-millibar level it is not surprising to find small but queer distortions in the forecast pattern. The use of the stream-function technique leads to a reduction in the root mean square contour height errors at 1000 millibars, but the synoptically unreal distortions of the contours usually preclude any improvement in the root mean square vector wind errors.

The effect of topography.—The numerical model used in the prolonged tests at Dunstable neglects the effect of topography, but it has been difficult to isolate specific errors due to this omission. The suspected topographical errors so far investigated have been mixed in indeterminate proportions with boundary effects and spurious anticyclogenesis. However, there is statistical evidence that the numerical predictions of 24-hour changes are at their worst over the parts of Greenland and central Europe within the verification area. So attempts are being made to include the effects of topography into the prediction model. Basically, the method is to superimpose on the parabolic vertical motion profile another plausible vertical velocity distribution with a ground-level value determined from the geostrophic flow and the broad-scale slope of the ground.[9]

A three-parameter model.—Early in 1960 a three-parameter model[10] was formulated for use with Meteor. The basic principles used to derive the Sawyer–Bushby model were applied to both the 1000–600 and the 600–200-millibar layers. In each layer the vertical motion profiles were parabolic with zero values at the 1000- and the 200-millibar levels and continuity at the 600-millibar level. The associated thermal wind assumption allows a change in thermal wind direction and speed per unit depth at the 600-millibar level. This three-parameter model was subjected to a four-month test early in 1960. The basic input and

output were similar to those for the two-parameter model with the addition of a 200-millibar contour field.

Figure 3 shows a 24-hour forecast derived with the three-parameter model. Examples of the computed vertical motion over four points on the chart illustrate the flexibility of the profiles imposed on the system.

Computed vertical motion and rainfall.—It is pertinent to inquire whether or not the computed vertical motion can be used to forecast rainfall. A rough and tentative answer to this question may be gleaned from contingency tables showing the relationship between observed rainfall and the sign of the computed vertical velocities. Table III shows the relationship between computed upward and downward motion and observed rainfall for four stations which are close to grid points of the computing network.

TABLE III—COMPUTED VERTICAL MOTION AND OBSERVED RAINFALL

Stations	Averages of computed vertical motion for 0000, 1200 and 2400 GMT using three-parameter model (1000–200 mb)	Observations at stations Rain	No rain
		percentage of cases	
Scilly, Elmdon, Stornoway, Tynemouth	UP	29	19
	DOWN	22	30

55 cases in the period 29 February to 10 June 1960.

Although it is fully realized that broad-scale vertical motion is only one of several important factors in the production of rainfall, the rather inconclusive relationships illustrated by Table III were disappointing. Several other forms of rainfall and vertical motion comparisons were tested, but with no better results. However, the result for the three-level model was at least better than that obtained for the two-level system; so it appears that the increased flexibility of the three-level model is at least an important step in the right direction.

Root mean square errors for the three-level model.—Tables IV and V show the root mean square contour height and vector wind errors for a set of 20 forecasts made by the Sawyer–Bushby model, the three-level model and C.F.O.

TABLE IV—ROOT MEAN SQUARE HEIGHT ERRORS IN 24-HOUR FORECASTS

	Two-level model	Three-level model	C.F.O.
	metres	*metres*	*metres*
1000 mb	94	72	71
500–1000 mb	68	58	67
500 mb	98	77	87
200–500 mb	62*	56	59
200 mb	114*	96	89

20 cases were used.

* Forecasts obtained by extrapolation upwards.

TABLE V—ROOT MEAN SQUARE VECTOR WIND ERRORS IN 24-HOUR FORECASTS

	Two-level model	Three-level model	C.F.O.
	metres	*metres*	*metres*
1000 mb	37	25	26
500–1000 mb	41	30	27
500 mb	31	28	30
200–500 mb	29*	28	25
200 mb	44*	34	31

20 cases were used.

* Forecasts obtained by extrapolation upwards.

These tables illustrate the superiority of the three-level model.

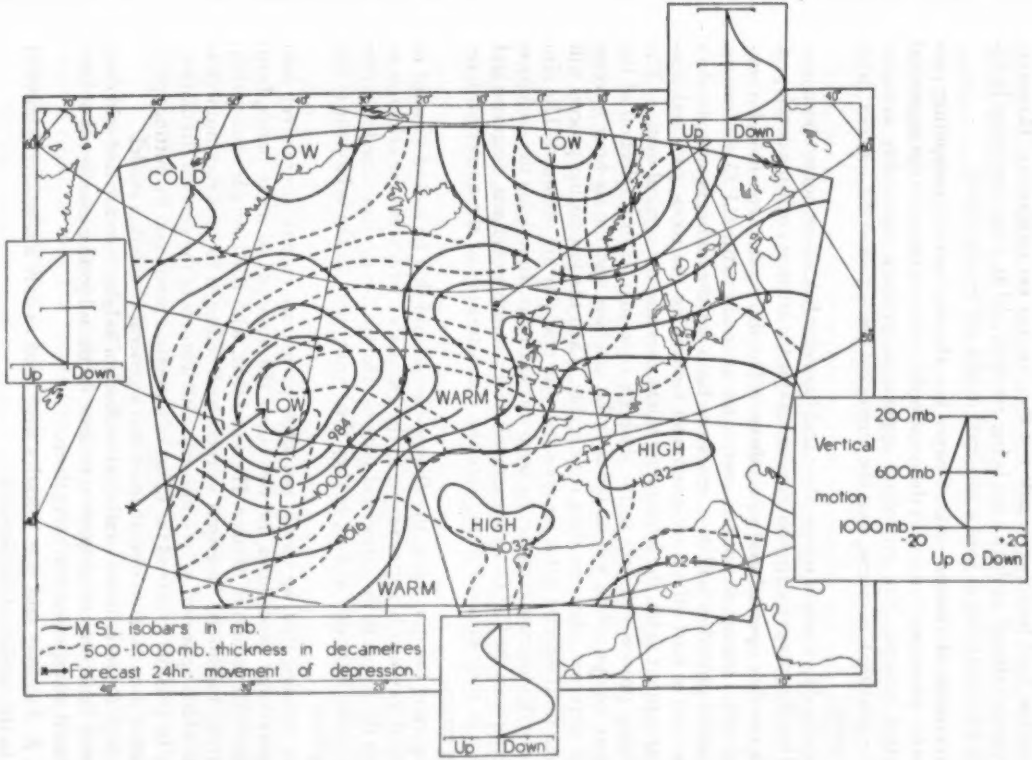


FIGURE 3—24-HOUR NUMERICAL FORECAST FOR 0000 GMT, 27 FEBRUARY 1959
USING THE THREE-LEVEL MODEL

The selected vertical motion profiles (applicable to the spots marked on the chart) are determined from the computed mean vertical speeds in the two layers 1000–600 and 600–200 millibars.

A hemispheric model.—The staff of the Dynamical Research Section (M.O.11) have started to explore the practicability of making forecasts on a hemispheric scale using Meteor. At present the principal problems arise more from computing rather than meteorological difficulties. Simple barotropic forecasts at the 500-millibar level have been made but they are not yet satisfactory. However, these computational problems are being resolved and at some later stage hemispheric forecasts using at least a two-level model will be attempted.

Movement of fronts.—Mr. Sawyer has already tested a computing procedure for advecting fronts with the geostrophic winds obtained in the numerical prediction process. The procedure appears to produce reasonably accurate results—provided, of course, that the numerical prediction of the contour fields is accurate.

Numerical forecasts of temperature and humidity fields have not yet been produced but the computing procedures for making the attempt are being developed.

The routine operational procedure.—The three-parameter model is considered useful enough to put into routine use as an aid to the C.F.O. forecasters. The routine procedure will start very soon. Each day the midnight synoptic data will be used to start off a three-parameter forecast for the 1000-, 500- and 200-millibar contour fields, the intervening thicknesses and the vertical motion. The forecasting process will stop at the six-hour time step, then a selection of the 0600 GMT synoptic data will be fed into the machine and the six-hour forecast will be corrected wherever data is available. Then the forecasting process will continue until forecast fields for 0600 GMT the next day are obtained. The aim is to have this forecast available by 0930 GMT each day. In judging the usefulness of each forecast the C.F.O. forecasters will have a number of charts, diagrams and statistics at their disposal to indicate the characteristics of the numerical forecasting errors.

In opening the discussion, the *Director-General* said that he looked forward to numerical forecasts being available to the Dunstable forecasters for criticism to root out the errors in the numerical systems. He was not too concerned at errors in particular forecasts, as long as they shed some light on the behaviour of the atmosphere.

After stressing that we should not allow premature empiricism to creep into our operational procedures, he welcomed the attempts to make hemispheric forecasts with the aid of Meteor. He noted that, with fairly simple assumptions concerning radiation, American workers had managed to construct numerical models which appeared to be consistent with features of the general circulation. It may be possible to extend the range of numerical prediction by putting specified rather than random disturbances into a general circulation model.

Mr. C. J. Boyden criticized statistical methods of judging forecasts and said that, at present there was no adequate substitute for the subjective assessment of forecasts based on experience in using them.

Mr. E. Knighting said that statistics were used as only a tentative numerical guide to the quality of the forecast.

Mr. N. E. Davis asked why the 200-millibar chart was chosen for numerical prediction, as the 200-millibar chart is a most difficult one to draw. The errors in radio-sonde heights are such that the chart cannot be drawn with any certainty. He suggested that ± 30 to 60 metres be added to all 200-millibar heights over the Atlantic and the numerical forecast based on these heights be compared

with that based on the original heights. If significant differences occurred then the conclusion would be that the 200-millibar chart was not suitable for numerical predictions.

Above 500 millibars, divergence increases to a maximum somewhere in the region of 300 or 250 millibars and then decreases again up to a level somewhere above 100 millibars, so that it would be preferable to use both the 300- and 100-millibar levels, though the 300-millibar level would be difficult to deal with, as it is the geostrophic motion in the vicinity of the jet stream that is the cause of development.

Mr. C. L. Hawson, referring to Mr. Davis's contribution, said there were indeed uncertainties in the 200-millibar analysis which are a handicap to any forecasting system. At C.F.O. some forecasters were now analysing the 100-millibar chart early, the 200-millibar chart being drawn in the light of the analysis at both 300 and 100 millibars. He believed the vertical velocity usually changed sign at some variable level above 300 millibars and after reaching another maximum, in summer at least, decreased to near zero at and above 100 millibars. He thought the 300-millibar surface more representative of the level of maximum divergence than the 200-millibar surface.

Mr. A. H. Gordon said that forecasters are particularly interested in the 1000-millibar chart and that he would therefore refer specifically to this level. Have comparisons of forecasts with actual charts been made on the basis of synoptic types, for example rapidly deepening depressions in their early stages, and polar or tropical lows? In fact, is all the dynamics included in the model equations as applied to these different systems? Then there are the large surges of pressure which are so difficult to forecast conventionally, yet which lead to changes of type, blocks, etc. Friction has not been mentioned in describing the model, but we all know that quite a few millibars can be gained or lost on account of friction.

With regard to spurious anticyclogenesis, could it be that the dynamics is there but that the spurious pattern is dynamically unstable so that the flow settles down and readjusts itself to the kind of pattern actually observed? Mr. Gordon also asked if there are any plans to extend the time scale to the 30-day CLIMAT period and to the infinite period of the general circulation.

Mr. M. K. Miles showed two cases of cutting-off which the two-level model failed to forecast. The first, made at 0000 GMT, 16 April 1959, showed that the trough was moved on too fast and contour heights at 500 millibars were not reduced sufficiently at the bottom of the trough or raised sufficiently in the narrowing neck over south-west England. Essentially the same errors were made in the forecast made at 0000 GMT, 10 September 1959.

In terms of cyclonic vorticity the actual charts differed from the forecast ones in having the maximum vorticity displaced towards the bottom of the trough with a region of very low values further north. One can suggest three main reasons for this difference:

(i) The numerical forecast started with an analysis at 500 millibars which had too little vorticity in the rear of the trough (that is, in the north-westerly flow);

(ii) the model was incorrectly advecting the existing vorticity; and

(iii) the model was neither producing new vorticity at the bottom of the trough nor destroying vorticity further north along the axis.

If a forecast based on a 500-millibar analysis containing the maximum amount of positive vorticity behind the trough consistent with the observations still showed this difference, then the forecaster would know that he should not expect the two-level model to predict the cutting-off process.

Mr. H. B. Rowles maintained that identification of the positions of jet streams from upper air charts is important and wanted to know to what extent this is possible on charts produced by numerical prediction. Does smoothing result in the loss of these features? The concentration of considerable wind errors in the Mediterranean mentioned by Mr. Wallington may well be due to the inadequate prediction of northward displacements of the subtropical jet stream in that region.

Dr. R. C. Sutcliffe said that he would like to encourage the study of individual cases of special interest, especially where forecasts have gone wrong in a qualitative way. The cutting-off process and, allied with this, the blocking development, are not yet satisfactorily explained in dynamical terms and quantitative calculations using different physical assumptions would be of great interest. There is limited scientific interest in minimizing statistical averages by trying this or that variation in the procedures—this savours of a reversion to empiricism. It may be necessary if the objective is to develop quickly the best system for regular practical use, but it is equally important and much more interesting to gain a scientific understanding of different types of dynamical development.

With the changes of organization associated with the move of the Meteorological Office to its new headquarters at Bracknell, more research effort will be devoted to the dynamical problems of long-range forecasting and the general circulation.

Mr. J. S. Sawyer said that blocking and cutting-off are large-scale processes not easy to get into the area used. Improving the model gradually seems the most satisfactory way of making progress at present.

In closing the discussion, the *Director-General* amplified his remarks on the need to acquire a better understanding of the atmosphere before expressing his thanks to Mr. Wallington for opening the discussion and to the various contributors.

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DUSTSTORM AT ADEN, 0830 GMT, 1 OCTOBER 1958

To face p. 89]

DUSTSTORM AT ADEN, 0830 GMT, 1 OCTOBER 1958

NOTES AND NEWS

A new housing for the sensitive cup anemometer, Mark I

The photograph facing page 69 shows a new housing for the electric contact type of the sensitive cup anemometer originally designed some twenty years ago by Professor P. A. Sheppard. The new housing has been designed by Dr. H. Charnock and Mr. F. E. Pierce of the National Institute of Oceanography to minimize the interference with the wind flow. By filling the instrument with carbon tetrachloride it can in its new form be used under water as a sensitive current indicator. For further details reference should be made to the article by Charnock and Pierce in the *Journal of Scientific Instruments*.[1]

REFERENCE

1. CHARNOCK, H. and PIERCE, F. E.; New housing for the sensitive cup-contact anemometer Mk. I. *J. Sci. Instrum., London*, **36**, 1959, p. 329.

CORRIGENDA

Variation of surface wind velocity with height in hilly terrain

The following amendments should be made to Table III on page 290 of the November 1960 *Meteorological Magazine:*

Delete the figures in the columns headed "Sept. 1959" and "Nov. 1958–Oct. 1959" and the last two rows of the column for "Feb. 1959" and *substitute:*

Sept. 1959	—	4·03	4·72	4·80	4·43	3·98	4·01	4·06	3·37	3·88	3·66	4·10
Nov. 1958–Oct. 1959	—	3·69	3·51	3·39	3·42	3·56	3·20	3·20	2·89	2·84	2·71	3·19
Feb. 1959											3·72	4·66